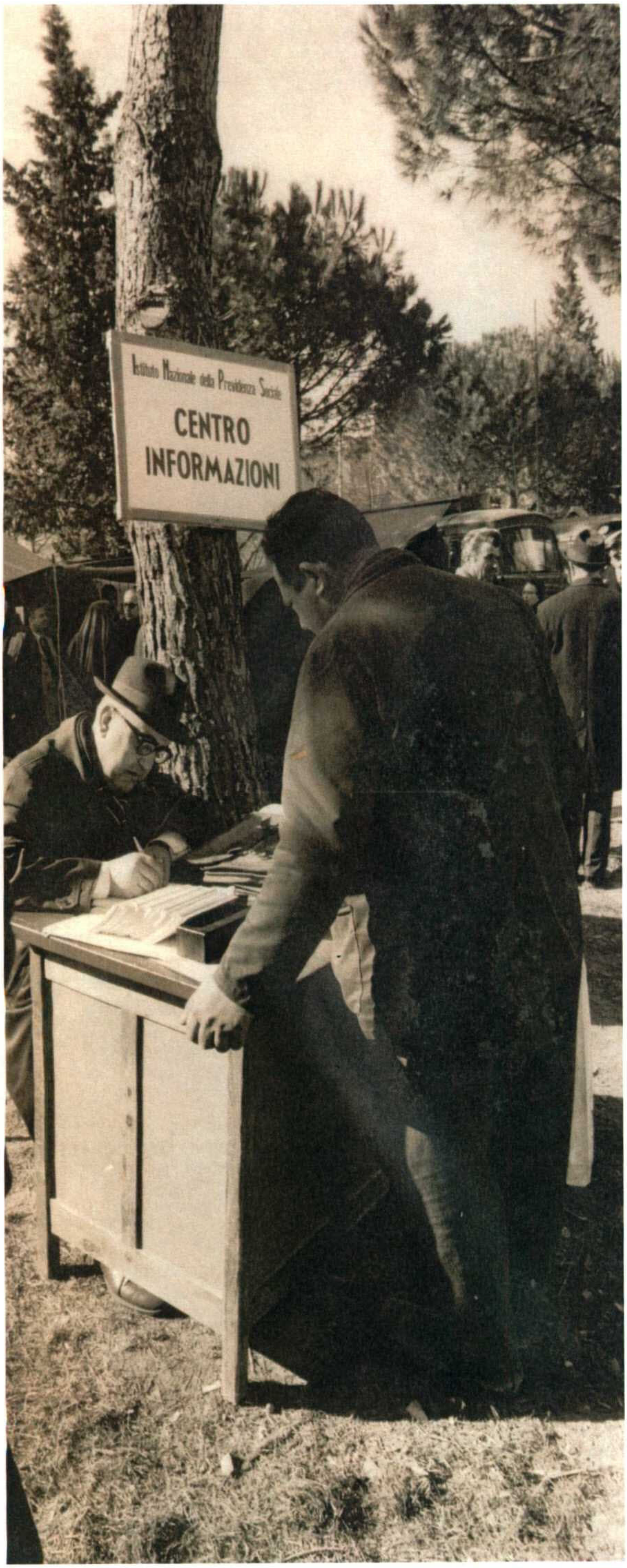

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A clash of generations? Increase in Retirement Age and Labor Demand for Youth

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Pietro Garibaldi

A clash of generations? Increase in Retirement Age and Labor Demand for Youth

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INDICE

Sintesi Non Tecnica

Il principale contributo di questo lavoro consiste nel valutare gli effetti sulle assunzioni di giovani della legge 214 del 2011 che, nel mezzo di una drammatica crisi finanziaria, ha bruscamente innalzato i requisiti anagrafici e contributivi per l'accesso alle pensioni. Vuole colmare un ritardo della letteratura empirica nel valutare le conseguenze di breve periodo sul mercato del lavoro di un innalzamento dell età di pensionamento. Il lavoro si basa su dati Inps sulle dichiarazioni contributive delle aziende, che permettono di meglio valutare la dinamica delle assunzioni in diverse fasce di età, e sulla ricostruzione delle carriere contributive dei singoli lavoratori, che permette di identificare i lavoratori bloccati in azienda dalla riforma e la durata di questi blocchi.

Qui forniamo una sintesi delle motivazioni e dei principali risultati del nostro lavoro. Dal 2010 ci sono in Italia 800.000 occupati in meno tra chi è sotto i 30 anni di età e 800.000 occupati in più al di sopra dei 55 anni. Non si tratta di un fenomeno attribuibile alla demografia, allo spostamento verso l' alto della gobba dei baby-boomers: il tasso di occupazione (il rapporto fra occupati e popolazione nelle diverse fasce di età) era praticamente uguale fra gli under 30 e gli over 55 all'inizio della crisi. Ora è al 45 per cento fra chi ha più di 55 anni e al 12% tra chi ne ha meno di 30. La Grande Recessione e la crisi dell' area Euro hanno portato con sè una riduzione di circa un terzo dell'occupazione tra i giovani, facendoci superare la soglia del 40% nel tasso di disoccupazione giovanile. Certo, questi sviluppi erano in parte prevedibili ed erano stati infatti previsti. In particolare, in presenza di un forte dualismo contrattuale giovani con contratti temporanei che possono essere interrotti dal datore di lavoro senza alcun onere, lavoratori anziani soggetti a regimi di protezione dell' impiego alquanto stringenti era legittimo aspettarsi una forte crescita della disoccupazione giovanile. E quanto avvenuto puntualmente in altri paesi a forte dualismo contrattuale, a partire dalla Spagna. Ma il dualismo contrattuale non può spiegare queste dinamiche così fortemente divergenti ai due estremi della distribuzione per età dell'occupazione, non può darci una ragione per la crescita delloccupazione al di sopra dei 55 anni di età.

Nel dicembre 2011, al culmine di una crisi finanziaria drammatica, il Parlamento italiano ha approvato una riforma pensionistica che, nel mezzo di una pesante recessione, ha bruscamente innalzato i requisiti anagrafici e contributivi per andare in pensione, allontanando la pensione fino a 5 anni per alcune categorie di lavoratori. Il quesito legittimo da porsi alla luce di questi sviluppi del mercato del lavoro e della riforma del 2011 è se e in che misura il brusco (e del tutto inaspettato) innalzamento dei requisiti per andare in pensione può avere avuto un effetto negativo sull'assunzione di giovani. Il quesito è rilevante anche per altri paesi che hanno adottato o stanno per adottare politiche di questo tipo per fronteggiare le conseguenze dell'invecchiamento della popolazione sui sistemi pensionistici. Se nel lungo periodo non ci sono ragioni per ritenere che un innalzamento dell'età di pensionamento possa avere effetti negativi sulle assunzioni di giovani, è possibile che, nel breve periodo, in un mercato del lavoro con regimi di protezione dell'impiego relativamente rigidi per i lavoratori anziani, queste politiche possano temporaneamente spiazzare il lavoro dei giovani. Sviluppiamo un semplice modello con generazioni sovrapposte e tre tipi di lavoratori (giovani, in età centrali e anziani) come guida per il lavoro empirico. Il modello ci dice che, di fronte a un'espansione forzata del numero di lavoratori anziani in un'impresa, vi può essere in effetti un calo delle assunzioni di giovani quando l'effetto negativo di scala domina leffetto di complementarità tra lavoratori giovani e meno giovani.

Per compiere la valutazione, abbiamo raccolto informazioni sull'universo delle imprese private con più di 15 dipendenti in Italia, utilizzando i dati dei flussi Uniemens sulle dichiarazioni contributive delle aziende. Dato che ci interessava analizzare l'andamento delle assunzioni di giovani prima e dopo la riforma, oltre che fra imprese che sono state investite in modo più o meno intenso dall' innalzamento dei requisiti, ci siamo concentrati su imprese che sono rimaste attive per l'intero periodo 2008-14. Si tratta di circa 80.000 imprese con una dimensione media di 70 addetti. In ciascuna impresa abbiamo potuto ricostruire se c'erano dei lavoratori bloccati dalla riforma e per quanti anni.

Abbiamo quindi comparato l'andamento delle assunzioni di giovani tra imprese diverse in quanto a numero di anni-lavoratore bloccati, controllando per le caratteristiche delle imprese (dimensione, settore, percentuale di operai e impiegati, composizione di genere, composizione per età della forza lavoro, salari medi dei giovani rispetto ai salari degli over 55, etc.). Le imprese con lavoratori bloccati hanno, in media, 11 anni-lavoratori di blocco. In tutte le analisi econometriche (condotte con metodo a doppie differenze, propensity score matching e rolling regressions sulla dimensione d' impresa) troviamo un forte effetto negativo dei blocchi sulle assunzioni di giovani e si tratta di un effetto statisticamente significativo. L'impatto dei blocchi è rilevante: 5 anni-lavoratore di blocco (ad esempio un lavoratore bloccato per 5 anni o due lavoratori bloccati per due anni e mezzo) comportano la presenza nell'impresa di un giovane lavoratore in meno. Proiettando questi risultati sull' insieme delle imprese con più di 15 dipendenti del settore privato, rimaste attive per tutto il periodo 2008-2014, abbiamo che i blocchi indotti dalla riforma del 2011, avrebbero ridotto le assunzioni di giovani di circa 37.000 unità. Si tratta di circa un quarto del calo delle assunzioni di giovani registrato in questo periodo.

I nostri risultati suggeriscono che innalzamenti dei requisiti pensionistici dovrebbero, se possibile, essere introdotti con una certa gradualità per evitare effetti negativi sul mercato del lavoro dei giovani. Le proprietà dei sistemi pensionistici che consentono una certa libertà ai lavoratori riguardo all' età di pensionamento, perchè rendono questa scelta neutra sul piano attuariale, possono essere utilizzate per attutire questi effetti senza pregiudicare i risultati di queste riforme nel ridurre la spesa pensionistica.

ABSTRACT

Most European countries experienced a dramatic increase in youth unemployment since the Great Recession of 2007-2009. For the Euro area as a whole, employment in the 15-24 age group declined by almost 17% over a 6 years span, in Southern Europe declines ranged between 34% (Italy) and 57% (Spain). Demographic and institutional developments cannot, by themselves, account for these dramatic changes in the structure of employment by age groups. This paper evaluates whether and to which extent the increase in the retirement age introduced in several countries in the middle of the recession could have contributed to divergent dynamics of employment rates at the two extremes of the age distribution. We take Italy as a case study as a major reform took place in December 2011 increasing the retirement by up to five years for some categories of workers. We have access to a unique dataset from the Italian social security administration (INPS) identifying in each private firm the fraction of workers hit by the increase in the retirement age. We look at the dynamics of youth hirings in the same firms as well as in firms where no workers were locked-in. Our results clearly indicate that before and after the reform, firms that were more exposed to the increase in employment duration of senior workers significantly reduced youth hirings. The results are also quantitatively sizeable. We estimate that a lock-in of five workers for one year reduces youth hiring of approximately one full time equivalent worker. Overall, out of a total loss of 150 thousand youth jobs, 36 thousand losses can be attributed to the reform. A variety of robustness tests confirm our findings.

Keywords: Pension Reforms, labor demand, lump-of-labor, youth unemployment.

1 Introduction

Most European countries experienced a dramatic increase in youth unemployment and a decrease in youth employment since the Great Recession of 2007-2009. While for the Euro area as a whole employment in the 15-24 age group declined by almost 17% in the 2007-13 period, in Southern Europe declines ranged between 34% (Italy) and 57% (Spain). The percentage decline in employment for the other age groups was way more modest: 3% for the Euro area as a whole, and in all countries between 1/3 and 1/6 of the employment decline for youngsters. At the other extreme of the age distribution, employment increased substantially. For the Euro area as a whole employment for people in the 55-65 age group increased by approximately 10 percent. Demographic developments played an important role in these developments, but cannot account, by themselves, for these dramatic changes in the structure of employment by age groups. Indeed, not only employment levels, but also employment rates of young and senior workers moved in opposite directions (Figure 1).

The strong increase of youth unemployment was predicted by the literature on contractual dualism. For instance, as suggested by Boeri and Garibaldi (2007) the honeymoon of youth unemployment following two-tier labor market reforms is followed by the nightmare of youth dis-employment as soon as macroeconomic conditions deteriorate. The large literature on contractual dualism, however, fails to explain divergent dynamics of employment rates at the two extremes of the age distribution.[1]

Several European countries increased the retirement age in the middle of the recession: Portugal in 2007, Spain in 2011, Greece in various stages between 2010 and 2016, and Italy in 2011. Is the divergent dynamics of employment rates at the two extremes of the age distribution related to these developments in retirement rules? Surprisingly enough, there is fairly little literature on the interactions between retirement and youth employment. The literature on retirement is typically focused on the supply side, and hence ignores trade-offs between young and older workers that may originate on the demand side.[2] There is some empirical literature on the age-productivity profile and on young-old substitutability, but its results are rarely framed in models of labor demand.

Italy provides an excellent case study to analyse the interactions between retirement rules and youth employment. Employment rates for 15-24 and 55-64 age groups were almost coinciding in 2005 (Figure 2). Ten years afterwards, the employment rate of the elderly is 45 percent while employment rate of the youth is approximately 12 percent. In this period the normal retirement age was increased and the minimum contribution requirements for access to early retirement tightened. In the middle of a run on the Italian public debt, a major reform took place in December 2011, imposed by markets and international organizations as Italy was contaged by the sovereign Euro debt crisis, increasing the retirement age by up to five years for some categories of workers. This policy change is now known as the ''Monti

[1] The pioneer work is Saint-Paul (1993). Boeri (2011) offers a survey of the literature up to the Great Recession. See Cahucet al. (2016), and Berton and Garibaldi (2012) for more recent work

[2] One notable exception is Vestad (2013) who uses administrative Norwegian data to estimate the impact of an early retirement program on youth employment using instrumental variable techniques.

Fornero reform". Estimating the impact of this mandatory increase in retirement on youth labor demand is the main contribution of the paper.

The Italian experience is valuable in addressing a broader issue, notably whether in labor markets driven by the demand side, as it is typically the case in the middle of a recession, unexpected increases in retirement age can have adverse effects on youth employment. We have access to a unique dataset from the Italian social security administration (INPS) on Italian firms before and after the reform. We look at whether a sudden and unexpected increase in the contributory and age requirements for retirement forcing firms to keep workers previously entitled to pensions to stay in the payroll, affects labor demand of the youth. We identify the population hit by the changes in retirement rules in each firm, and look at the dynamics of net hirings in the same firms. Our results are very clear, and indicate that before and after the reform, firms that were more exposed to the mandatory increase in the retirement age significantly reduced youth hirings.

The results are also quantitatively sizeable. We show that a block of five workers for one year reduces youth hiring by approximately one unit. Overall, out of a total loss of 150 thousand youth jobs, 36 thousand losses can be attributed to the reform. Our results survive to a variety of robustness checks, including rolling regressions across the size distribution, propensity score matching and a falsification test on the pre reform years.

The paper first briefly surveys the existing literature. In section 3 we provide a conceptual framework to look at the age structure of labor demand. We propose a simple labor demand problem with labor of different age groups interacting within the firm. The economics of a pension reform and labor demand is more subtle than a simple exogenous shift in labor supply, since most of the individuals involved are already employed and can not be easily fired. We thus call a pension reform a *forced expansion at the firm level.* Overall, there are two effects at work. First, there is a negative scale effect due to decreasing returns to scale. The reform forces some of the old workers to stay employed rather than retire. Even though this tends to increase output, with decreasing marginal returns to scale in production the marginal product of young workers falls and so does youth hiring. Second, there is an effect that depends on the degree of complementarity between young and old worker. The question is ultimately empirical. It thus may well be that transitional pension reforms increasing the retirement age are not a good time for youngsters. In section 4, we describe in some details the pension reform that took place in Italy in December 2011. In section 5 we describe the data, spell out the empirical strategy, and provide the basic estimates. Section 6 performs the various robustness checks while section 7 concludes, and points out basic policy implications.

2 Literature review

As stressed above, the literature on retirement schemes is typically focused on the supply side, and often neglects the labor demand side. Yet, there are two strands of the existing literature that are particularly relevant for our work. The first has to do with the relationship between age and productivity. The second with tests of the so-called *lump of labor fallacy.*

Research on the age-productivity relationship has to find proper measures of age-specific

Figure 1: Employment Rate of Youth and Old in EU 15

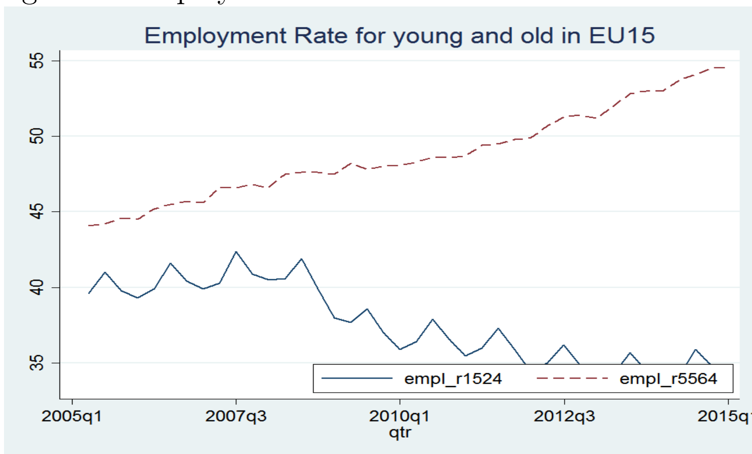
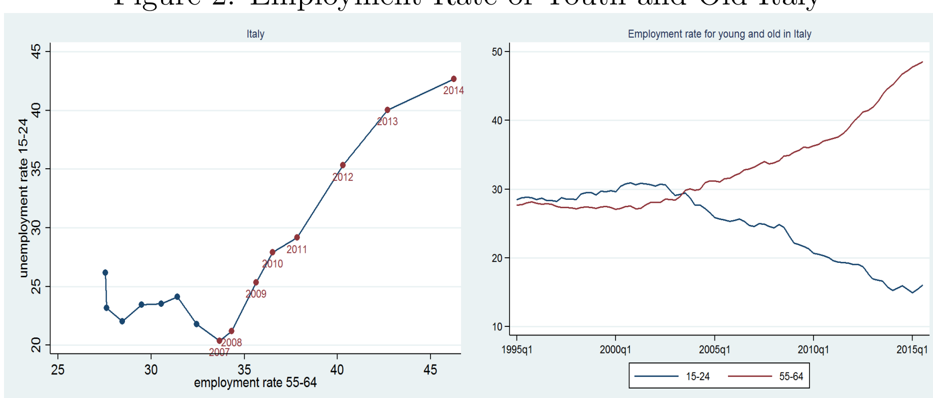


Figure 2: Employment Rate of Youth and Old Italy



productivity, and often relies on perceptions of employers. For instance, Barth et al. (1993) reports that, according to employers, older workers have higher health care costs and lower flexibility in accepting new assignments, and they may be less suitable for training. Older workers are also considered to be more consistent, cautious, slow, and conscientious. Johnson (1993) reports that most employers believe in a rule of thumb that average labor productivity declines after some age between 40 and 50. Remery et al. (2003) assessing employers' opinions about ageing in the Netherlands, finds that employers are less favorable—higher wage costs, lower productivity—about older workers the higher the share of older workers in the firm, which may hint at complementarities between young and older workers.

Quantitative assessments of the age-productivity profile often rely on cross-sectional variation (Warr, 1998) at the plant level[3] . For instance, Borsch-Supan et al. (2006), based on a case study in a large manufacturer of cars in Germany, do not find that productivity declines with age. Errors are more frequent with older workers, but are also less severe than

[3]See Garibaldi et al. (2011) for a review of this literature

those made by young workers. Avolio et al. (1990) find that tenure is a better predictor of work performance than age, in jobs with high complexity. Vandenberghe (2013) finds that a larger share of older workers in a firm does not affect gross profits. Cross-sectional studies, however, cannot control for cohort effects. Indeed, it is often found (see Boeri and vanOurs (2013)) that the variance in performance is greater *within* age groups than *between* age groups.

More recent studies used matched employer-employee data to assess the relationship between age, productivity and wages. This research points to a crucial role of institutions, notably wage setting and employment protection. For instance, Hellerstein et al. (1999) using a U.S. matched worker-firm data found that for prime-aged workers and older workers, productivity and earnings increase at the same rate over the life cycle, while Crepon et al. (2003) using the same methodology on French data found that older workers are relatively overpaid. The age profile of wages has a concave pattern, while the age profile of productivity stops rising and even decreases after some experience level. They conclude that a policy of raising the normal retirement age may be problematic because of the poor performance of older workers in the labor market. Ilmakunnas and Maliranta (2005), using Finnish firm data with matched average worker characteristics, similarly found that the wage-productivity gap increases with age. Dostie (2006), based on Canadian linked worker-firm data, found that both wage and productivity profiles are concave, but productivity diminishes faster than wages for workers aged 55 or older.

While many economists, based on sound economic principles, challenge the popular belief that there is a fixed number of jobs, notably a *lump of labor* that may be redistributed from older workers to hires of young people costlessly, not much empirical work has been done on this issue. Boldrin et al (1999) document a positive cross-country relationship between youth unemployment and the retirement age. Boeri and vanOurs (2013) likewise report a negative cross-country relationship between the employment rates of older workers (aged 55–64) and the unemployment rates of young workers (aged 20–29). Needless to say, these findings may capture a long-run relationship between retirement age and youth employment/unemployment. In this paper, we focus on the short-run effects of a reform suddenly and steeply increasing the retirement age, and we use firm-level evidence.

3 The labor demand effects of temporary pension reforms

Technology and Population Dynamics

A representative firm produces with two inputs, labor N and capital K. In the long run, both N and K are flexible, and the production technology is characterized by a production function $F(N, K)$ that is quasi-concave and exhibits constant returns to scale. As we focus on labor demand and abstract from capital, we assume that the production function can be written as $y = f(N) \equiv F(N, K)$ for some K. It follows that $f'(N) > 0$, $f''(N) < 0$. With the Cobb-Douglas formulation we can then write $y = AN^{\alpha}$, with $A = K^{1-\alpha}$.

The labor force consists of young, prime aged, and old workers. Let L_1, L_2 and L_3 denote the number of young, prime aged and old workers, respectively. We assume that the number of efficiency units of labor they produce is given by $N = g(L_1, L_2 + aL_3)$, where the aggregator g exhibits constant returns to scale. Hence the prime aged and the old workers are perfect substitutes, but may supply different numbers of efficiency units of labor. We typically think that $a < 1$, in order to be coherent with the evidence on the age productivity profile surveyed above. The basic idea is that old and prime aged workers are closer substitutes than old and young.

Total output is thus $y = f(g(L_1, L_2 + aL_3))$. Denote the partial derivatives of g with g_1 and g_2, respectively, both strictly positive in the entire domain. We require that the composite function $\tilde{f}(L_1, L_2) \equiv f(g(L_1, L_2))$ is strictly concave. At this stage we do not put restrictions on g_{12}, and our general specification is thus coherent with a technology that features complementarity as well as substitution between young and senior workers.

In each period, a unit mass of workers is born and die. Workers are risk netural and maximize the present discounted value of their income stream. Workers that are young in period t, are prime aged in period $t+1$ and old in period $t+2$. Thereafter they die.

Labor Demand and Market Equilibrium

As we focus on the demand side, we assume that all workers who are entitled to retire do so. In the initial equilibrium, all older workers retire and individuals spend at most 2 periods in the employment relationship. A young worker who is hired in period t will be prime-aged in period $t+1$. If there are no adjustment costs nor employment protection legislation (EPL), the firm can adjust L_1 and L_2 freely in each period, and the problem is static. If there is EPL for prime-aged (and possibly older) workers, then the firm cannot costlessly fire workers. If EPL is extreme- as we assume - the firm faces the constraint that $L_2^{t+1} \geq L_1^t$. Hence the firm's problem is inherently dynamic, and can be solved as a Kuhn-Tucker problem with a set of complementary slackness conditions. For a representative firm, for which the constraints are not binding, the first order conditions are[4]

$$f'(N)g_1(L_1, L_2) = w_1 \tag{1}$$

$$f'(N)g_2(L_1, L_2) = w_2 \tag{2}$$

where $N = g(L_1, L_2))$. Market clearing requires that $L_1 = L_2 = 1$, and this determines wages w_1 and w_2. Note that in any period, the representative firm's stock of prime workers are the young employees in the previous period. Since the wages of prime aged workers are equal to their productivity, the representative firm on the margin is indifferent between retaining them or not, and will retain them. Hence the representative firm only hires young workers. As prime aged workers cannot be fired, the firm will have to renew the contract

[4] Note that market equilibrium ensures that the interior maximum for the representative firm is exactly at the constraint, hence the associated Kuhn-Tucker multiplier is zero.

to all the workers who were young in the previous period. In the pre-reform period, this constraint does not bind.

Employment effects of reforms

We now consider a pension reform that increases the retirement age for one period only. The retirement age of the youth will thus not change and they will retire regularly after 2 periods. At a first glance, this reform is akin to a temporary increase in labor supply. As a matter of fact, the economics of such a pension reform- from the labor market standpoint- is more subtle than a simple exogenous shift in labor supply, since most of the individuals involved are already employed and cannot be easily fired. We thus call a pension reform a *forced expansion at the firm level.*

Consider now that the government, unexpectedly, and for one period only, labeled T, requires that the firm employs ΔL older workers. With flexible wages, it follows straightforwardly that the wages of both young and prime aged workers will adjust so that demand again equals supply. Wages for prime aged workers will certainly fall, while the wages for young workers may fall or increase, as the discussion below highlights.

Assume instead that wages are fixed in the period the shock occurs: this assumption is key in order to obtain the employment effects that follow. Furthermore, due to EPL, the firm cannot fire prime-age or older workers. We assume that wages are flexible in period $T+1$. Hence the marginal productivity of prime-age workers in that period is equal to their wage. As a result, the continuation value of hiring a young worker in period T in period $T+1$ is zero.[5] By the same equilibrium argument outlined above, it follows that the firm's period T hiring decision of young workers in effect is a static maximization problem. It follows that[6]

$$\begin{aligned} \frac{dL_1}{d\Delta L} &= -\frac{f'(N)g_{12} + f''(N)g_1 g_2}{f''(N)g_1^2 + f'(N)g_{11}} \\ &= k\,[f'(N)g_{12} + f''(N)g_1 g_2] \end{aligned} \tag{3}$$

where $k = -1/(g_1^2 f''(N) + f'(N)g_{11}) = -1/\tilde{f}_{11} > 0$. Equation (3) represents our key theoretical prediction on the effect of a temporary retirement reform. A forced expansion has two effects. The first term in (3) captures the effects of the *degree of complementarity* between young and old workers. If $g_{12} > 0$, more old workers will increase the marginal productivity of young workers, and this will tend to increase the amount of hiring of new

[5]Whether the wage of prime-age workers is flexible, or fixed and the firm is under EPL, does not influence the marginal productivity of young workers, neither does the wage for older workers. Since the reform is for one period only, the wage for older workers will not influence the allocation of resources: it is just a transfer between the firm and the older workers, and is therefore ignored.

[6]Taking derivative of the first order condition (1) gives

$$f''(N)g_1[g_1\frac{dL_1}{\Delta L} + g_2] + f'(N)[g_{11}\frac{dL_1}{\Delta L} + g_{12}] = 0$$

workers. The second term reflects a negative effect due to *decreasing returns to scale in production*. As the reform forces some of the older workers to stay employed rather than to retire, this will, ceteris paribus, increase output. Since there are decreasing returns to scale in production, this will negatively affect the marginal product of young workers and hence reduce hiring.

Simple Examples

Consider first the case where young and older workers are perfect substitutes, and write $g(L_1, L_2) = a_1L_1 + a_2L_2$. It follows that $g_1 = a_1$, $g_2 = a_2$, and that $g_{12} = g_{11} = g_{22} = 0$. It follows that

$$\frac{dL_2}{\Delta L} = -a_2/a_1 \tag{4}$$

In this case, increasing the number of older workers employed reduces the number of young workers, efficiency unit by efficiency unit. At the other extreme, if the production technology is Leontief, an increase in the number of older workers employed increases the scope for employment of the young proportionally.

Suppose now that $f(N) = AN^\alpha$ as above, and that g is a CES production function, $g(L_1, L_2) = (L_1^\rho + L_2^\rho)^{\frac{1}{\rho}}$. Let $l = L_0/L_1$, the fraction of the employees that are young initially. It follows that

$$\frac{dL_1}{\Delta L} = -\frac{l(\rho - \alpha)}{1 - \rho + (1 - \alpha)l^\rho} \tag{5}$$

Hence the number of young workers employed decreases with L_2 whenever $\rho > \alpha$. Note that if $\rho = 1$, the inputs are perfect substitutes, and (5) implies that $\frac{dL_1}{d\Delta} = -1$. Finally, suppose that g is Cobb-Douglas, $g(L_1, L_2) = L_1^\beta L_2^{1-\beta}$. In addition, $f(N) = AN^\alpha$ as before. In this case,

$$\frac{dL_1}{d\Delta L} = \frac{\alpha\beta}{1 - \alpha\beta} l > 0 \tag{6}$$

Hence, with this specialization of the production function, an increase in L_2 always increases L_1.

Heterogeneity

Suppose that firms have the same production technology, but vary in terms of their capital stock K. Since both F and g exhibit constant returns to scale, so does the composite function. Hence we can write the first order conditions (1) and (2) as functions of K and the intensities, $l_i = Li/K$, $i = 1, 2, 3$,

$$F_1(g,1)g_1(l_1,l_2) = w_1 \quad (7)$$
$$F_1(g,1)g_2(l_1,l_2) = w_2 \quad (8)$$

Trivially, l_1 and l_2 are independent of k. Now suppose that the reform forces the firm to increase its labor force by ΔL_2 units. Since $\Delta l_2 = \frac{\Delta L_2}{K}$, it follows that l_2 increases by $\Delta L_2/K$ units. Let $\gamma \equiv \frac{dl_1}{dl_2}$.[7] It follows that γ is independent of K. Since $\frac{dL_1}{dL_2} = \frac{dl_1}{dl_2} = \gamma$, we can write $\Delta L_1^i = \gamma \Delta L_2$, independently of K (everything else being equal, one would expect ΔL_2 to be proportional to K).

In our model, all firms are in a steady state. When the reform hits, one may expect different firms to be in different states, some being on an expansionary path, and some on a contractionary path. Firms that plan to expand in the period in which the reform hits, and hire more prime-age workers, have the possibility of accommodate the supply shock by hiring fewer prime-age workers than planned. For those who can accommodate fully, and obtain an optimal stock of L_2 workers after the shock, the effect on the hiring of young workers should be negligible (provided that old and prime-age workers are perfect substitutes). Other firms may be able to accommodate some of the shock by cutting back on new hirings as much as they can, and thereby reduce the effect somewhat, while other firms that do not plan to hire prime-age workers at all will take the full blow of the reform. This discussion suggests that if we have a cross section of firms i subject to a forced expansion driven by a temporary pension reform, we want to estimate the following relationship

$$\Delta L_0^i = \gamma \Delta L_2^i \quad (9)$$

where $\Delta^i L_2$ is the number of locked-in workers in firm i. Note that the sign of γ can be positive or negative, depending on the relative size of the complementarity effect and the scale effect. The answer to this question is empirical. Finally, since we also consider the variation in youth employment at given wages, and other firm specific variables, a more general relationship is

$$\Delta L_0^i = \gamma \Delta L_2^i + \beta X^i \quad (10)$$

where X^i is a vector of firm specific variables. Note that a negative sign of the γ coefficient implies a short run crowding out youth labor demand by the pension reform.

4 The 2011 pension reform in Italy

In the middle of the European sovereign crisis, in November 2011 Mario Monti became prime minister of Italy. His Government enacted in December 2011 a bold reform steeply increasing

[7]Equivalent to (3), γ can be written as

$$\gamma = -\frac{F_1(n,1)g_{12} + F'(n)g_1g_2}{F'(n,1)g_1^2 + F'(n,1)g_{11}}$$

contributory and age requirements to obtain an early retirement or old age pension. This reform was unanticipated and dictated by the need to restore confidence in Italian public finance after the interest on long-term Government bonds had reached an historical peak at 7.56% in the government auction of November 29, 2011. The sovereign crisis that hit Italy in the Fall of 2011 was both repentine and intense, and the fall of the Berlusconi government was unlikely to be envisaged by Italian firms. In addition, it was far from obvious what would happen after the fall of the Berlusconi government. As the financial crisis unfolded, events took place over a very few days. Giorgio Napolitano, the President of the Italian Republic, appointed Mario Monti as life senator on November 9, 2011. The Berlusconi government resigned on the hands of the President on November 12, and Mario Monti received the mandate to form a new government on November 13. He swiftly put together a technocrat government that took office on November 16. On December 4, the pension reform was approved, alongside a package of other austerity measures in a rescue package named "Save Italy". As the reform was enacted as a Government decree it become immediately effective. It is now known as the Fornero-Monti reform, named after the Labor Minister in office in the Monti government.

The contribution required to be eligible for early pensions was increased by up to 5 years as the previous system of so-called *quotas* (combining seniority in contributions and age requirements) was replaced by a pure contribution requirement and gender differences were removed. Table 1 provides details as to changes in old age retirement rules before and after the reform for the public sector. Old age pensions were also increased, notably for women, in the public sector and in self-employment, whose age requirements were increased by up to 3 years. All these changes were to be effective one month later, at the beginning of 2012.

At the same time, the reform kept the flexibility in the retirement age for the cohorts of workers entered in the labor market after 1996 and subject to the new notionally defined contributory (NDC) system. Thus, the increase in the age requirement was bound from the very start to be temporary, allowing for greater flexibility in the retirement age as the cohorts entered in the labor market in 1996 would age up to reaching the range of retirement ages allowed by the new system, which is itself indexed to life expectancy.

The reform also involved an acceleration of the transition to the NDC system, forcing every worker to enter the new system on a flow basis. A lower indexation to price inflation of pensions was also introduced. Overall, the reform was supposed to involve cumulative savings of 80 billion between 2012 and 2021 (Inps, 2013), approximately 5 percent of GDP.

As the reform was completely unexpected, it involved many casualties. Among these some 100,000 workers who had agreed to voluntarily leave a job in the context of collective bargaining agreements in the understanding that they would have drawn a pension. The Government had to intervene with 7 safeguard measures in the following years for a cumulative cost to date of about 12 billions to (partly) fix this problem.

Table 1: Changes in Retirement Age for Regular Pension in December 2011

	Requirements ante reforms			Requirements post reform Fornero	
year	age limit men	age limit women	mobile window	age limit men	age limit women
2011	65	60	12 months		
2012	65	60	12 months	66	62
2013	65 e 3 months	60 e 3 months	12 months	66 e 3 months	62 e 3 months
2014	65 e 3 months	60 e 4 months	12 months	66 e 3 months	62 e 9 months
2015	65 e 3 months	60 e 6 months	12 months	66 e 3 months	62 e 9 months
2016	65 e 7 months	61 e 1 months	12 months	66 e 7 months	65 e 7 months
2017	65 e 7 months	61 e 5 months	12 months	66 e 7 months	65 e 7 months
2018	65 e 7 months	61 e 10 months	12 months	66 e 7 months	
2019	66	62 e 9 months	12 months	67	
2020	66	63 e 3 months	12 months	67	

5 Data and empirical strategy

We draw on data extracted from the Italian social security (Inps) archives, tracking all dependent workers in the private and public sectors as part of the collection of contributions earmarked to pensions and social insurance. The dataset that was assembled for this analysis tracks all private firms with more than 15 employees in 2011 that had been operating without discontinuities in contribution records between 2008 and 2014 in Italy. The final database comprises almost 80,000 firms. Each firm is observed in three different years, notably 2008, 2011 and 2014, corresponding to a dataset of 240,000 cells.

The unit of observation is the individual employer responsible for the payment of social security contributions to Inps. In 96 per cent of the cases this unit corresponds to a firm. In the remaining 4 per cent these units belong to the same group. We do not have access to the records of each individual worker, but we know the average characteristics of workers in these firms (age, gender, blue-collar or white-collar position, fixed-term or open-ended contract). As Inps knows the contribution seniority of each individual worker, we could also establish how many workers in each firm have been locked-in by the 2011 reform. We also know if there are in each firm workers (as well as how many) who were later on involved in one of the safeguard measures mentioned above.

For each firm we observe the total number of employees, the (one digit) sector of operation, the region, the number of part time employees, the number of blue and white collars, the number of temporary contract workers. We also have information on the the age distribution of employees, in particular the number of employees aged less than 30 and more than 50. Finally we know the average wage overall and for young and older workers as well as average earning of white and blue collar workers.

A $locked-in$ worker is a senior worker whose retirement rule has been postponed in December 2011. Formally, a locked-in worker is an employee aged 55 or more, whose retirement age has been suddenly postponed in December 2011.

The variable *locked-in* will play a key role in our empirical analysis. Table 2 reports the distribution of locked-in workers by firm size. Approximately 50,000 firms- corresponding to 64 percent of firms-had no locked in workers in 2011. The rest of the firms had at least one locked-in worker, with the percentage of firms with multiple numbers of locked in workers rapidly declining.

A firm with a positive number of locked-in workers is a firm that is treated by the Monti Fornero reform. We thus have approximately 30,000 firms that are treated with different intensity and 50,000 firms that were not affected by the reform. In our regressions, we are interested in the intensity of the treatment imposed to each firm. We thus measure the intensity of treatment in terms of the person-years locked-in, labeled *locked-in-year* in 2011. A *locked-in-year* equal to 5 means that a firm had a cumulative locking of five years in 2011, obtained as combination of number of workers and number of years (e.g., one worker locked-in for five years, or five workers for one year, etc.). All firms for which there are no locked-in workers have a level of *locked-in-year* equal to zero. Between 2008 and 2011, *locked-in-year* has a value o zero for all firms

5.1 Empirical Strategy

The main empirical question is thus to estimate the impact of the variable *locked-in-year* on youth hiring between 2011 and 2014.

While we can confidently argue that the policy shock was exogenous and unanticipated, what is less obvious is that firms with no locked-in workers are statistically identical to those with positive locked-in workers. The potential selection bias in the intensity of the treatment will be the core concern of our robustness checks.

We define $young_{it}$ as the number of young workers below the age of 30 in firm i at time t. Note that $young_{it}$ includes both temporary and permanent workers.[8]. The dynamics of $young_{it}$ evolves according to

$$young_{i,t+1} = young_{i,t}(1 - \delta_i) + h_{i,t} - s_{i,t}$$

where h_{it} is gross hirings between t and $t-1$, $s_{i,t}$ is gross separations, and $\delta_i * young_{i,t}$ are the number of youth workers that pass the age threshold between time t and time $t+1$. Our basic variable of interest is the gross change $dyoung_{i,t+1}$, as the change in youth employment between t and $t+1$ and thus read

$$dyoung_{i,t+1} = h_{i,t} - s_{i,t} - \delta_i * young_{i,t}$$

where $dyoung_{i,t+1}$ includes also the reduction in employment due to a pure age passing effect. When we restrict the analysis to permanent workers, the variable is labelled $dyoungperm_{i,t+1}$. Figure 3 reports the scheme of our empirical strategy. The three vertical lines indicate the

[8]Within the temporary workers, we include any form of employment within the firm, including so called *contratti a progetto*, a category of workers that de facto act as consultant within the firm, but are dependent employees in any effect

time points in which we observe the firms. The intensity of treatment variable is the *locked_in_ years* in 2011, a variable that measure the person-year of locked-in workers inside the firm. The key outcome is youth hirings. Our baseline specification considers only data between 2011 and 2014, and is a simple cross section on employment differences

$$dyoung_i = \alpha + \beta X_i' + \gamma * locked\_in\_year_i + \epsilon_i \tag{11}$$

where X_i' is a vector of variables for firm i, and $locked\_in\_year_i$ are year-workers locked-in for firm i in December 2011. γ is the coefficient of interest, and $\gamma < 0$ implies that locked-in workers reduce youth hiring. We will consider different controls and different versions of the left-hand-side variables. In particular, we can control for the size of the firms, the wage, and most importantly the share of older worker within the firm. When we also want to consider pre treatment differential trend, we estimate the following regression including 2008 observations

$$dyoung_{it} = \alpha + \beta X_{it}' + \delta t + \gamma * locked\_in\_year_{it} + \epsilon_{it} \tag{12}$$

where t is a dummy variable taking value of 1 if year is 2011. Note that in the specification (12), $locked\_in\_year_{i,t} * t = locked\_in\_year_{i,t}$, since all firms with no locked in workers have a value of $locked\_in\_year_i$ equal to zero. In some specifications, in order to exploit fully the panel version of our dataset, we consider the model in levels and use firm fixed effects so that,

$$young_{it} = \alpha_i + \beta X_{it} + \gamma * locked\_in\_year_{it} + \epsilon_{it} \tag{13}$$

Note that since locked-in-years between 2011 and 2008 is zero, the coefficient γ of equation 13 can be estimated in first differences, and the interpretation of the γ estimate in equation (11) and (13) is identical.

5.2 Descriptive statistics

Table 3 reports some descriptive statistics on the change in youth employment between the firms with positive locked-in workers and firms with no locked-in workers. Since our variable *locked_in_year* is numerical, in Table 3 we are bunching all firms with a positive number of locked-in workers in one group. In this section, we call such group the treatment group, but in the regressions what is captured is the intensity of treatment.

Employment in the group with positive locked_in_workers fell in the 2011-14 period by 3.25 employees on average compared with 1.42 workers in the control group. Note that the change in 3.25 workers is accounted for by 2.84 by permanent workers and 0.42 by temporary workers. Table 3 also reports the average value of the control variables in 2011-2014. Table 3 clearly indicates that firms in the control groups experienced, on average, a marked decline in youth employment already in the period 2008-2011, while youth employment was slightly increasing in the treatment group. This violates any common trend assumption. We address this issue below.

Figure 3: Fornero Monti Reform as a natural experiment

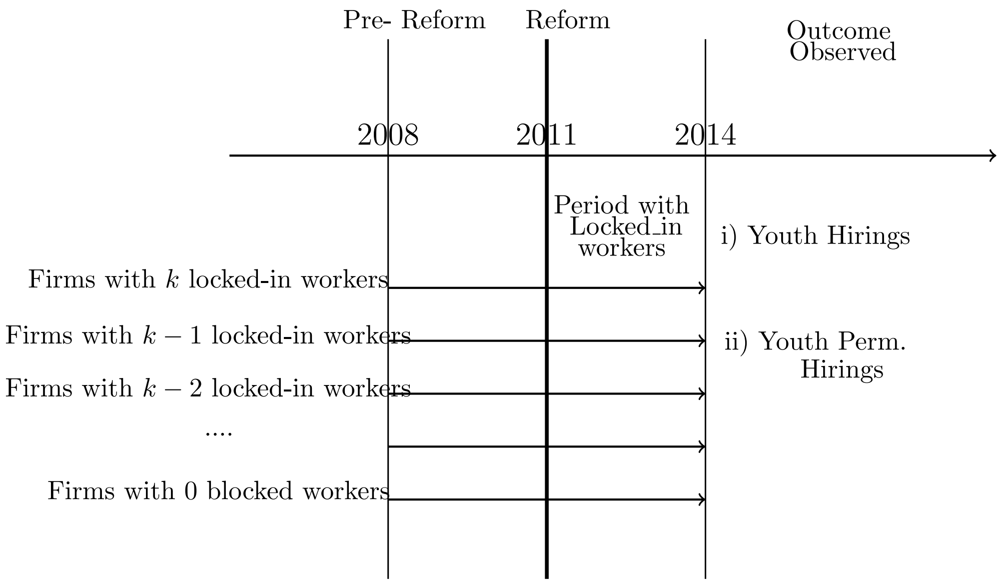


Table 4 reports summary statistics on the key covariates. Unsurprisingly, firms with workers locked-in are, on average, larger than firms in the control group. Conditional on having at least one worker locked-in, we can look at the distribution of the incidence of locked-in workers on total dependent employment in each firm (Figure 4). Table 4 shows also that firms in the treatment groups have a higher average wage than firms in the control groups. This wage differential is linked to the well known firm-size firm-wage effect (Brown-Medoff, 1989), a stylized fact in empirical labor economics. Note that the wage differential holds for the average wage, the wage of open ended employees (*wperm*), and of young as well as older permanent job holders. Old wage refers to the wage of workers beyond the age of 50 while young wage refers to the wage of workers below the age of 30. Table 4 reports also information on the ratio of young to old wage workers. There seems to be a larger wage compression across the age structure in the control group (young to old wage is 0.8 in the control against 0.65 in the treatment group), even though such a difference is milder in the post treatment period.

Note that in all the regressions that we perform, we restrict the sample to firms with less than 671 employees, which represents the 99th percentile of the distribution of firms by number of employees. The reason why the very large firms are excluded from the analysis rests on the fact that as size increases, the probability of hiring a locked-in worker tends to 1. Very large firms have always some workers that are locked-in. Moreover, most of the large units in our data capture employers belonging to the same group, and hence do not fit well into the optimization problems characterized in the previous section.

5.3 Estimation and quantitative effects

The basic idea behind using the Monti Fornero reform as a natural experiment is reported in Figure 3. As described above, firms are observed in three different data points, corresponding to average youth employment in 2008, 2011 and 2014. We thus have two observations per firm of variations in youth employment, where the 2011-2014 period corresponds to the treatment period. The same structure holds regardless of the intensity of treatment, as indicated in Figure 3, for the control group.

The basic regressions of the change in youth employment on *locked_in_year* are reported in Table 5. The γ coefficient is reported in the first row. It is negative and significant in all specifications. From column (1) to column (5), additional regressors are added. In column (2), we include the size of the firms, measured by the number of workers, both linearly and with a quadratic term. A non-linearity coherent with a concave relationship appears significant in all specifications, as the quadratic term for size is always negative. Note that from column (3) onwards we control also for the share of older workers within firms. As one would expect, a large increase in the wage of older workers is negatively associated with youth hirings.

The magnitude of the coefficient γ on *locked_in_year* is fairly stable and around -0.12 to -0.13 in different specifications. Its standard deviation is also stable. The elasticity of the change in youth employment with respect to *locked_in_year*. evaluated at the average values, is $\epsilon_\gamma = \frac{\hat{\gamma} * \overline{locked\_in\_year\_i}}{\overline{dyoung_i}}$ where $\overline{dyoung} = -2.06$ and $\overline{locked\_in\_year\_i} = 3.93$ are the observed average values in the sample. The implied ϵ_γ elasticity is thus 0.23, which suggests that the effect is quite sizeable. Every five locked-in workers-years are associated with the loss of one youth job. In the overall sample, the total number of youth jobs lost is 164 thousand. Evaluated at the average elasticity, the results in Table 5 imply an average loss of 36 thousands workers associated to the Monti Fornero reform.

5.3.1 Alternative Specifications and Outcomes

In Tables 6 we perform the rest of the regressions of equations (11) and (13). Table 6 adds 2008 observations and controls to the simple regression of Table 5. The size of the coefficient γ tends to become lower than 0.10 when the 2008 controls are added. In Table 6, the elasticity ϵ_γ falls to 0.18. The time dummy is always negative and significant, suggesting a sharp reduction in 2011-2014 for the entire sample. The γ coefficient represents, in this case, the additional effect due to the locked-in workers.

Table 7 performs a basic fixed effect estimate of γ. The estimate is stable across specifications, and suggests that $\hat{\gamma} = 0.16$ with a ϵ_γ estimated to be 0.3, 20 percent higher than the estimates obtained in the basic specificaiton of Table 5. The resilience of our negative estimate to a firm fixed effect specification is remarkable. While it is true that the size of the γ in the fixed effect is lower than in the baseline specification, we do not take the value of 0.073 to be our most precise value, as the robustness regression with propensity score matching will show below.

Table 8, 9 and 10 perform similar sets of regressions but with a different outcome, mainly the change in open ended youth employment. Table 8 uses a simple diff-in-diff estimate with observations on 2011 and 2014. Table 9 adds 2008 controls while Table 10 performs fixed effects regressions. If we define by γ_{perm}, the coefficient on *locked-in-years* in Tables 8, 9 and 10, we see that the estimates are always negative and significant. In the specifications that add most controls, the estimate of γ_{perm} varies between -0.0972 in Column (5) Table 8, to -0.0572 in Column (6) of Table 9. When we run the fixed effect estimate in Table 10, the coefficient on *locked_in_year* is -0.147 in Column (5), a value not different from the basic specification of Table 5

6 Robustness

While the estimates of the *locked_in_year* coefficients γ and γ_{perm} are stable across specifications, even when firm fixed effects are considered, the summary statistics provided in Table 4 require a variety of robustness checks. We have two main concerns. First, we know that firms with positive *locked_in_year* are statistically different- in terms of observables and firm size in particular- to firms with zero value of the *locked_in_year*. Firm size plays a key role in this respect. Second, firms with positive *locked_in_year* did destroy youth jobs also in the pre-treatment period, as indicated by Table 3.

In the rest of this section we perform three robustness checks. First, we match the samples by a set of propensity score methods, and we then run the basic regression on the matched sample. Second, we run a battery of rolling regressions by dividing the sample in terms of homogeneous group size of firms. Finally, we run rolling placebo regressions on the 2008-2011 period, when the reform was not in place yet, and the variable *locked_in_year* can not measure any direct policy measure.

6.1 Propensity Score Matching

The propensity score matching is meant to adjust for pre-treatment observable differences between the treated firms and untreated firms (Abadie and Imbens, 2006 and Abadie et al., 2004). In this exercise we thus combine (or match in the technical jargon) a group of firms treated by the reform with a group of non treated firms with similar observable characteristics. The control group is then used to estimate the unobservable (contrafactual) outcome

The variables we use for matching the two samples are a size category variable that distinguishes firms between small, medium, and large, the sector, the share of older workers, the share of blue and white collar workers, and the share of women. While the simple matching would apply to category variables describing the presence of locked-in workers inside the firm, the treatment variable we are mainly interested is the number of workers-years summarized by *locked_in_year*, which explicitly accounts for the intensity of the treatment. In Table 11 we report the mean value of the various variables according to match characteristics. Note that the mean of the variables in the two groups is not statistically different. Overall, the matching is certainly successful, even though matching on some variables (oldshare in particular) falls below conventional significance values.

Table 12 performs the simple linear regression of equation 11 using the matched sample. Interestingly enough, the size of the coefficients γ on *durata* is -0.142, a value that is not too distant from the first simple set of equations that we run in Table 5. This result is very important. When the treatment and the control groups become homogeneous in terms of observable variables, the impact of *locked-in-years* on youth employment change is similar to the simple OLS estimate of Table 5.

6.2 Rolling Regressions

The second robustness check deals with rolling regressions in terms of firm of similar size categories. The idea of the exercise is to divide the sample of firms into cells of at least 100 observations, and run a regression of *dyoung* on *locked_in_year* and other controls on the sub cells. The controls we use are blue collar and white collar shares, older worker share, women share and totworkers. We end up with 212 cells of similar size categories. Table 13 reports basic summary statistics on these 212 regressions, while Figure 5 reports the value of all the 212 γ coefficients in the regressions.

The average value of γ is negative and equal to -0.19. In addition, 200 regressions out of 212 display a negative γ coefficient, and 65 percent of them are significant at the 10 percent level. This clearly suggests that the effect is fairly robust across the entire size distribution.

6.3 Placebo Rolling Regressions

The last set of robustness checks is based on placebo regressions. The idea is to run the rolling regressions of Table 13 on the period 2008-2011, when the reform was not in place and there was not such a thing as workers locked in. In this falsification exercise we impute to each firm the value of the *locked_in_year* variable observed in 2011 also to the previous period, and we take the regressor as a placebo, since any effect we may observe can not be due to the reform. The summary statistics are reported in Table 13. The average value of the $\gamma_{placebo}$ regression coefficient is -0.09 negative, but half as large as the effect estimated for the period 2011-2014.

In Figure 6 we report the values of the $\gamma_{placebo}$ coefficient across the entire size distribution. The number of regressions with a negative γ falls from 200 in Figure 5 to 167 in Figure 6. Most importantly, only 78 regressions display a significant coefficient, indicating that 2 out of 3 estimated γ coefficients are not different from zero. Overall, it is clear that the reform effect was negative and significant, even though some of the destruction of youth jobs did start before the reform in those same firms that were subsequently hit by the reform.

7 Conclusions and Policy Implications

The paper investigates the relationship between a temporary increase in retirement age and youth labor demand. In a simple model of labor demand and different ages of workers as inputs, a temporary pension reform increasing the retirement age has two effects. First, there is a negative scale effect due to decreasing returns to scale. The reform forces some of the old workers to stay employed rather than retire. Even though this tends to increase output, with decreasing marginal returns to scale in production, the marginal product of young workers falls and so does their hiring. Second, there is an effect that depends on the degree of complementarity between young and older workers.

Ultimately, the contribution of the paper is empirical. The experience of the Italian pension reform in the middle of the 2011 sovereign debt crisis provides a perfect setting for testing the relationship between youth labor demand, and the increase in the retirement age. The bold pension reform was unanticipated and repentine, and happened in the middle of an aggregate recession. The paper has access to a unique dataset drawn from the Italian Social Security Administration that identifies at the firm level the intensity and the number of workers locked-in by the increase in retirement. The data set covers all private sector firms with more than 15 employees between 2008 and 2014. The cross sectional variation in the firms' exposures to the mandatory delay in retirement, allows us to estimate the impact of locked-in workers on youth hiring at the firm level. The simple difference-in-difference regressions with the intensity of treatment given by the person-years locked-in, as well as a variety of robustness checks, show that the effect of the increase in retirement had a sizeable negative impact on youth labor demand. Our estimates suggests that 23 percent of the youth employment loss in the private sector between 2011 and 2014 in firms with more than 15 employees can be accounted for by the Monti Fornero reform. In aggregate numbers, out of 160 thousands youth job losses, 36 thousands can be imputed to the reform.

The policy implications of our results should be drawn with great caution. Nevertheless, we can make two points. First, reducing the generosity of pensions in the middle of the European sovereign crisis was probably inevitable, despite the severe recession that Southern European economies were experiencing. But this tightening could have been engineered by reducing pension levels of those retiring before the normal retirement age, and hence allowing firms to encourage the exit of the least productive older workers. With an hindsight, as well as with the scientific evidence provided in the paper, we also feel that much more should have been done by European policy makers to help and sustain young workers who were about to enter the labor market in the same years. The odd "old in-young out" equilibrium in which Southern European labor market entered in the last decade, is unlikely to be the desired outcome, and the risk of a lost European generation is certainly there. Second, the retirement age should be as flexible as possible. As far as Italy is concerned, the long-run DC system will ensure a viable and sustainable system. Yet, such a system has a prolonged transition phase. Along this medium run adjustment to the new system, policy attempts to increase flexibility in retirement in an actuarially neutral fashion should be taken extremely seriously into account.

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Table 2: Share of Firms with blocked workers in December 2011

blocked	Freq.	Percent	Cum.
0	50,954	64.42	64.42
1	16,164	20.44	84.86
2	5,667	7.16	92.02
3	2,496	3.16	95.18
4	1,202	1.52	96.70
5	710	0.90	97.60
6	452	0.57	98.17
7	304	0.38	98.55
8	214	0.27	98.82
9	180	0.23	99.05
10	136	0.17	99.22
11	112	0.14	99.37
12	84	0.11	99.47
13	82	0.10	99.58
..	...	....	....
22	10	0.01	99.97
23	18	0.02	99.99
24	9	0.01	100.00
Total	79,093	100.00	

Table 3: Average Changes in Youth Employment and *locked_in_worker*

	Pre-Treat* 2008-2011	Pre-No_lock_in ** 2008-2011	Post-Treat* 2011-2014	Post-No_lock_in ** 2011-2014
dyoung[a]	-1.75	0.46	-3.25	-1.42
dyyoungperm[b]	-1.55	0.11	-2.84	-1.01
dyoungtemp [c]	-0.21	0.35	-0.42	-0.42
locked_in_worker_year [d]	0	0	10.98	0
N	28439	50954	28439	50954

All variables refer to youth below age of 30

* Treatment refers to the bunching of all firms with positive *locked_in workers*

** *No_lock_in* refers to the bunching of all firms with positive *locked_in workers*

* Treatment refers to the bunching of all firms with positive *locked_in workers*

[a] employment change regardless of contract

[b] employment change among open ended contract

[c] employment change with temporary contract

[d] intensity of treatment as *locked_in_worker*

Table 4: Descriptive Statistics for Firms with and without locked_in_workers

	Pre-Treat* 2008-2011	Pre-No_lock_in ** 2008-2011	Post-Treat 2011-2014	Post-No_lock_in ** 2011-2014
totworkers [a]	144.2	37.28	141.7	37.70
menshare [b]	0.652	0.617	0.651	0.615
womshare [c]	0.348	0.383	0.349	0.385
blueshare [d]	0.575	0.572	0.556	0.560
whiteshare [e]	0.361	0.360	0.376	0.377
w [f]	28255.1	23793.1	29509.4	25071.9
wperm [g]	27550.7	22656.4	28893.1	24225.5
wyperm [h]	16607.0	14238.9	15396.9	13333.3
woperm [i]	32996.8	18454.7	32893.9	22603.4
wyoungold [j]	0.632	0.827	0.542	0.609
N	28438	50954	28438	50954

* Treatment refers to the bunching of all firms with positive *locked_in workers*

** *No_lock_in* refers to the bunching of all firms with positive *locked_in workers*

[a] Total number of workers; [b] Average share of men

[c] Average share of women ; [d] Share of blue collar

[e] Share of white collar; [f] Average wage

[g] Average wage of permanent contract

[h] Average wage of youth with permanent contract

[h] Average wage of old with permanent contract

[h] young wage over old wage

Table 5: Basic Specification on *dyoung*

VARIABLES	(1) dyoung	(2) dyoung	(3) dyoung	(4) dyoung	(5) dyoung
locked_in_year	-0.119***	-0.124***	-0.137***	-0.134***	-0.134***
	(0.0295)	(0.0389)	(0.0435)	(0.0433)	(0.0432)
totworkers		0.0146***	0.0144***	0.0129***	0.0133***
		(0.00327)	(0.00331)	(0.00332)	(0.00331)
totworkers2		-3.23e-05***	-3.11e-05***	-2.87e-05***	-2.92e-05***
		(8.59e-06)	(8.62e-06)	(8.64e-06)	(8.65e-06)
oldshare			3.262***	3.625***	3.531***
			(0.644)	(0.643)	(0.639)
dwageo				1.54e-05***	1.57e-05***
				(2.11e-06)	(2.12e-06)
dwagey				3.99e-05***	4.03e-05***
				(2.73e-06)	(2.77e-06)
Lblueshare					2.551***
					(0.666)
Lwhiteshare					2.520***
					(0.732)
Lwomshare					0.272***
					(0.103)
Constant	-1.362***	-1.841***	-2.093***	-2.075***	-4.557***
	(0.0724)	(0.0822)	(0.0914)	(0.0916)	(0.639)
Observations	78,540	78,540	78,540	78,540	78,540
R-squared	0.009	0.012	0.013	0.016	0.017

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Basic Specification on *dyoung* with 2008 observations and time dummy

VARIABLES	(1) dyoung	(2) dyoung	(3) dyoung	(4) dyoung	(5) dyoung	(6) dyoung
locked_in_year	-0.119***	-0.0993***	-0.0958***	-0.0792***	-0.0782***	-0.0786***
	(0.0295)	(0.0306)	(0.0309)	(0.0284)	(0.0284)	(0.0284)
t_2014	-1.234***	-1.271***	-1.271***	-0.292***	-0.302***	-0.248***
	(0.0812)	(0.0798)	(0.0787)	(0.0739)	(0.0739)	(0.0785)
totworkers		0.00563*	0.00571*	0.000322	0.000303	0.000206
		(0.00337)	(0.00336)	(0.00334)	(0.00337)	(0.00333)
totworkers2		-2.40e-05**	-2.42e-05**	-2.42e-05**	-2.42e-05**	-2.41e-05**
		(1.16e-05)	(1.16e-05)	(1.16e-05)	(1.16e-05)	(1.16e-05)
oldshare			-0.995***	1.511***	1.581***	1.432***
			(0.362)	(0.354)	(0.347)	(0.339)
dwageo				1.17e-05***	1.20e-05***	1.20e-05***
				(1.44e-06)	(1.45e-06)	(1.45e-06)
dwagey				4.60e-05***	4.62e-05***	4.63e-05***
				(2.14e-06)	(2.23e-06)	(2.27e-06)
Lblueshare					1.221	
					(0.879)	
Lwhiteshare					1.252	
					(0.951)	
Lwomshare					0.886***	
					(0.0760)	
blueshare2008						0.173
						(0.259)
whiteshare2008						0.128
						(0.295)
womshare2008						0.811***
						(0.0761)
Constant	-0.128***	-0.236**	-0.160	-1.105***	-2.582***	-1.540***
	(0.0394)	(0.109)	(0.119)	(0.119)	(0.919)	(0.315)
Observations	157,107	157,107	157,107	149,436	149,436	149,436
R-squared	0.009	0.011	0.011	0.014	0.015	0.014

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Panel Specification on *young*, Fixed Effects

VARIABLES	(1) young	(2) young	(3) young	(4) young
locked_in_year	-0.222***	-0.195***	-0.178***	-0.160***
	(0.00888)	(0.0104)	(0.0103)	(0.0101)
totworkers		0.179***	0.179***	0.186***
		(0.00419)	(0.00419)	(0.00503)
totworkers2		-4.71e-05***	-4.68e-05***	-5.45e-05***
		(1.38e-05)	(1.38e-05)	(1.49e-05)
oldshare			-6.910***	-5.772***
			(0.258)	(0.257)
woperm				-1.31e-05***
				(1.39e-06)
wyperm				-5.98e-06***
				(1.95e-06)
Constant	7.305***	-1.255***	-0.759***	-0.771***
	(0.00706)	(0.149)	(0.150)	(0.171)
Observations	235,794	235,794	235,794	228,057
R-squared	0.015	0.370	0.373	0.341
Number of id	78,807	78,807	78,807	78,754

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Basic Specification on *dyoungperm*

VARIABLES	(1) dpermy	(2) dpermy	(3) dpermy	(4) dpermy	(5) dpermy
locked_in_year	-0.103***	-0.0924***	-0.100***	-0.0981***	-0.0972***
	(0.0258)	(0.0305)	(0.0336)	(0.0335)	(0.0334)
totworkers		0.00724***	0.00711***	0.00565**	0.00599**
		(0.00253)	(0.00255)	(0.00256)	(0.00255)
totworkers2		-2.28e-05***	-2.20e-05***	-1.97e-05***	-2.01e-05***
		(6.46e-06)	(6.48e-06)	(6.49e-06)	(6.49e-06)
oldshare			2.053***	2.392***	2.284***
			(0.490)	(0.486)	(0.481)
dwageo				1.12e-05***	1.15e-05***
				(1.57e-06)	(1.58e-06)
dwagey				4.13e-05***	4.20e-05***
				(1.85e-06)	(1.92e-06)
Lblueshare					3.002***
					(0.622)
Lwhiteshare					3.096***
					(0.678)
Lwomshare					0.302***
					(0.0818)
Constant	-1.050***	-1.264***	-1.422***	-1.388***	-4.346***
	(0.0631)	(0.0625)	(0.0688)	(0.0692)	(0.594)
Observations	78,540	78,540	78,540	78,540	78,540
R-squared	0.012	0.014	0.015	0.019	0.021

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Basic Specification on *dyoungperm* with 2008 observations

VARIABLES	(1) dpermy	(2) dpermy	(3) dpermy	(4) dpermy	(5) dpermy	(6) dpermy
locked_in_year	-0.103***	-0.0722***	-0.0714***	-0.0576**	-0.0565**	-0.0572**
	(0.0258)	(0.0246)	(0.0252)	(0.0235)	(0.0234)	(0.0234)
t_2014	-0.652***	-0.721***	-0.721***	0.00890	-0.00730	0.0359
	(0.0700)	(0.0639)	(0.0637)	(0.0600)	(0.0599)	(0.0642)
totworkers		0.00118	0.00120	-0.00301	-0.00301	-0.00316
		(0.00302)	(0.00301)	(0.00303)	(0.00307)	(0.00302)
totworkers2		-1.91e-05*	-1.92e-05*	-1.91e-05*	-1.91e-05*	-1.89e-05*
		(1.06e-05)	(1.06e-05)	(1.07e-05)	(1.07e-05)	(1.07e-05)
oldshare			-0.230	1.459***	1.441***	1.432***
			(0.284)	(0.277)	(0.270)	(0.279)
dwageo				8.06e-06***	8.35e-06***	8.16e-06***
				(1.13e-06)	(1.14e-06)	(1.13e-06)
dwagey				4.56e-05***	4.61e-05***	4.58e-05***
				(1.62e-06)	(1.74e-06)	(1.74e-06)
Lblueshare					1.894**	
					(0.868)	
Lwhiteshare					2.045**	
					(0.937)	
Lwomshare					0.652***	
					(0.0606)	
blueshare2008						0.0613
						(0.235)
whiteshare2008						0.135
						(0.267)
womshare2008						0.529***
						(0.0622)
Constant	-0.397***	-0.310***	-0.293***	-0.962***	-3.013***	-1.234***
	(0.0333)	(0.0970)	(0.105)	(0.107)	(0.908)	(0.292)
Observations	157,107	157,107	157,107	149,436	149,436	149,436
R-squared	0.007	0.011	0.011	0.018	0.020	0.019

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Panel Specification on *youngperm*, fixed effects

VARIABLES	(1) permy	(2) permy	(3) permy	(4) permy	(5) permy
locked_in_year	-0.147***	-0.189***	-0.170***	-0.157***	-0.147***
	(0.00840)	(0.00767)	(0.00848)	(0.00844)	(0.00840)
totworkers	0.125***		0.123***	0.123***	0.125***
	(0.00398)		(0.00321)	(0.00321)	(0.00398)
totworkers2	-2.83e-05**		-2.62e-05**	-2.60e-05**	-2.83e-05**
	(1.14e-05)		(1.07e-05)	(1.07e-05)	(1.14e-05)
oldshare	-4.396***			-5.174***	-4.396***
	(0.201)			(0.200)	(0.201)
woperm	-1.11e-05***				-1.11e-05***
	(1.15e-06)				(1.15e-06)
wyperm	9.39e-06***				9.39e-06***
	(1.69e-06)				(1.69e-06)
Constant	0.117	5.699***	-0.239**	0.133	0.117
	(0.135)	(0.00610)	(0.114)	(0.114)	(0.135)
Observations	228,057	235,794	235,794	235,794	228,057
R-squared	0.267	0.018	0.298	0.301	0.267
Number of id	78,754	78,807	78,807	78,807	78,754

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Matching on Observables

	Mean			Test		
Variable	Treated	Control	bias	t	$p > \|t\|$	$V(T)/V(C)$
sizecat	1.8094	1.8812	-9,1	283.74	0.000	0.99
settore	4.0871	3.952	5.2	40.69	0.000	1.09
oldshare	.10921	.09957	11.3	141.16	0.000	0.81*
blueshare	.51012	.5758	-19.0	-40.21	0.000	1.11*
womshare	.46205	.35721	36.5	-21.52	0.000	0.99
whiteshare	.42269	.36722	17.5	14.65	0.000	1.08*
* if variance ratio outside [0.93; 1.07]						
Ps R2	LR chi2	p¿chi2	MeanBias	MedBias	B	R
0.079	2903.67	0.000	16.5	14.4	2.0	1.03
* if$B > 25\%$, R outside [0.5; 2]						

Table 12: Regression with Matched sample by Propensity Score

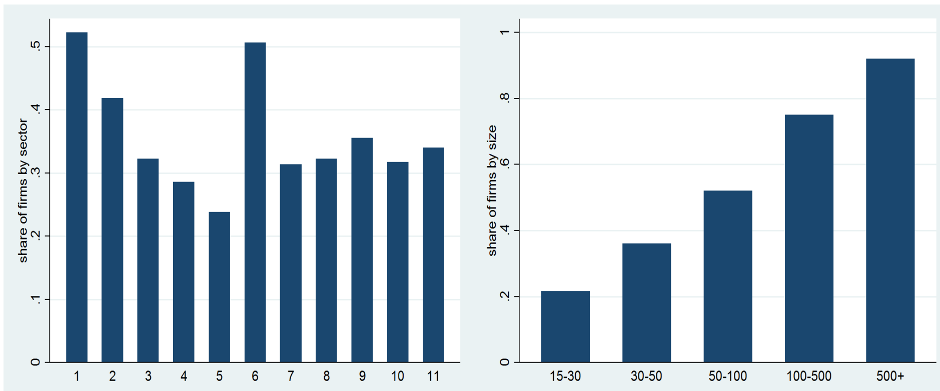
VARIABLES	(1) dyoung
locked_in_year	-0.142***
	(0.00166)
sizecat	-0.826***
	(0.0325)
settore	0.273***
	(0.0108)
oldshare	9.958***
	(0.245)
blueshare	-0.712***
	(0.188)
womshare	0.398***
	(0.103)
whiteshare	-3.038***
	(0.214)
Constant	0.0337
	(0.201)
Observations	462,566
R-squared	0.031

Dependent variable is change in youth employment, r regardless of contract
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Summary Statistics on Rolling Regressions

Variable	Regressions	Mean	Std. Dev.	Min	Max
$\gamma_{locked\_in\_year}$	212	-.1950317	.1898069	-1.029677	.4631037
$\gamma_{placebo}$	212	-.0900956	.1550128	-.5286243	.6158376

Figure 4: Share of Firms by Sector and Size



1= agriculture and fishing; 2= manufacturing; 3= constructions; 4= transportation and trade, accommodation and food service activities ; 5= information and communication ; 6= financial and insurance activities; 7= real estate; 8= professional, scientific, technical activities; 9= public defence, education, human health and social work; 10= arts and recreational activities; 11= extra-territorial organisations

Figure 5: Rolling Regression

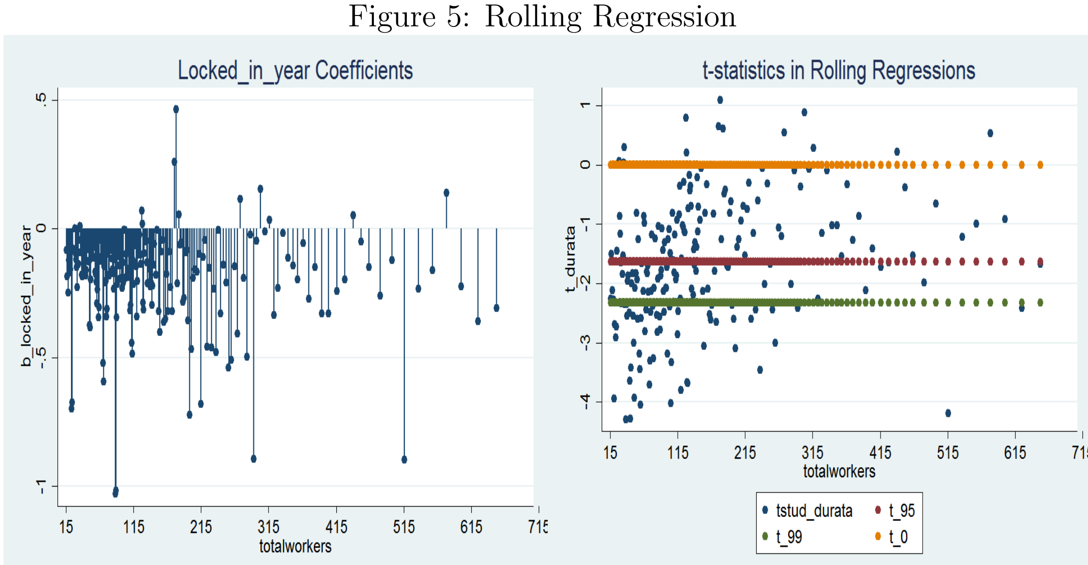


Figure 6: Placebo Rolling Regression